PRELIMINARY VERSION: COMMENTS AND CRITICISMS WELCOME

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MODELS, HIGH-ENERGY THEORETICAL PHYSICS AND REALISM

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ABSTRACT

Examples of theory development in quantum field theory and in S-matrix theory are related to three questions of interest to the philosophy of science. The first is the central role of highly abstract, mathematical models in the creation of theories. Second, the process of creation and justification actually used make it plausible that a successful theory is better characterized as being stable against attack rather than as being objectively correct. Lastly, the issue of the reality of theortical entities is discussed in light of this representation of theory generation and selection.

1. INTRODUCTION

Today I shall use developments in the field of theoretical high-energy physics to examine three issues of interest to the philosophy of science. First will be the use of highly abstract, formal models in the generation and evolution of theories. Second, I shall argue for a representation of the scientific enterprise in which there is no sharp separation between the creation and justification processes and in which the objective - subjective (or perhaps rational - sociological) distinction is blurred. Finally, the question of the reality of theoretical entities will be related to these first two issues. More details than I can give here are available in extended papers (Cushing, 1982a, 1982b).

Although it has been fairly common (Wartofsky, 1980, pp. 1-5) until recently to treat the evaluation and discovery aspects of science as largely disjoint undertakings, detailed case studies of past scientific achievements do reveal certain patterns of discovery and also indicate that discovery and justification are coupled processes (Rantz, 1980; MacKinnon, 1980; Nickles, 1980). This is especially true in the early stages of a research program. Ed MacKinnon (1980, p.169) has made a very useful distinction among three stages in the development of a scientific theory: formation, deductive unification, and axiomatic reformulation. Even though these activities are not always completely separate ones, I shall be focusing largely on the early stage of the creation of new theoretical frameworks. An important tool in this process of theory construction is the use of models. Mary Hesse (1966) has argued for the role of models and analogies in scientific theories. It appears that there has been a significant evolution in the use of models

away from models of analogy (in Hesse's sense) in nineteenth century physics toward much more formal and abstract analogies in current theoretical physics (Cushing, 1982a). Such models form a bridge between one theory and another and between the discovery and justification processes. This has been indicated by Michael Redhead (1975) who has seen the form of a mathematical theory as often being the invariant persevering through theory change as the terms in the theory are reinterpreted or redefined. One can see, for instance, that in the transition from classical to quantum mechanics the form of the equations of motion (that is, Hamilton's equations) remained the same although the meaning of the dynamical variables changed markedly. Redhead has also identified the central use of theoretical models and their stretching or development via exploration of their excess mathematical content (1980a) and the prevalence of these methods in particle physics (1980b). As with Born and Heisenberg, so in modern particle theory, a specific problem solution has often served as a model for subsequent work. Rantz (1980) has also recognized this in his chemical bonding case study. Models (often of formal mathematical analogy) serve as a means of embedding a theory in a mathematical framework. This latter framework when studied in detail or expanded reveals an unexpectedly rich structure which can in turn be used to extend the theory. Models are central for this fitting and stretching process. Quantum mechanics abounds with these surplus structure results, such as the Bohm-Aharonov (1959) effect. This extended theory is then taken seriously enough to investigate its empirical consequences. Elie Zahar (1979, 1980) has written on

the creative role of mathematics in the discovery of physical theories. I (Cushing, 1982a) have studied explicity the use of models in current high-energy theory and have found a predominant shift toward the use of highly formal mathematical models. This trend toward formalism as the origin of physical theories was presaged by Dirac in his famous 1931 paper on magnetic monopoles.

> "The steady progress of physics requires for its theoretical formulation a mathematics that gets continually more advanced. This is only natural and to be expected. What, however, was not expected by the scientific workers of the last century was the particular form that the line of advancement of the mathematics would take, namely, it was expected that the mathematics would get more and more complicated, but would rest on a permanent basis of axioms and definitions, while actually the modern physical developments have required a mathematics that continually shifts its foundations and gets more abstract. Non-euclidean geometry and non-commutative algebra, which were at one time considered to be purely fictions of the mind and pastimes for logical thinkers, have now been found to be very necessary for the description of general facts of the physical world. It seems likely that this process of increasing abstraction will continue in the future and that advance in physics is to be associated with a continual modification and generalisation of the axioms at the base of the mathematics rather than with a logical development of any one mathematical scheme on a fixed foundation.
> "There are at present fundamental problems in theoretical physics awaiting solution, e.g., the relativistic formulation of quantum mechanics and the nature of atomic nuclei (to be followed by more difficult ones such as the problem of life), the solution of which problems will presumably require a more drastic revision of our fundamental concepts than any that have gone before. Quite likely these changes will be so great that it will be beyond the power of human intelligence to get the necessary new ideas by direct attempts to formulate the experimental data in mathematical terms. The theoretical worker in the future will therefore have to proceed in a more indirect way. The most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalise the mathematical formalism that forms the existing basis of theoretical physics, and after each success in this direction, to try to interpret the new mathematical features in terms of physical entities."

Dirac then cited his own theory of the relativistic electron whose negative-energy states subsequently became identified with the positron. This need to go from mathematics and theory to experiment in current physics, rather than the reverse route which had been predominant in the the past, has also been commented on by C. N. Yang (1980). Although I shall be listing several examples of this process later, let me just mention here quantum electrodynamics, renormalization, black holes, non-abelian gauge fields and, possibly, even Dirac's own magnetic monopole (Cabrera, 1982).

To make a transition to some of the sociological aspects of science let me parody one view of science. Science is seen as a basically rational enterprise in which objective truth is sought and discovered (Pietschmann, 1976). Of course, there are social factors present, but these are extraneous to the essence of science. Such factors may be necessary since humans are involved in science, but surely they are undesirable for and extraneous to the scientific enterprise. These influences are to be eliminated from the final, objective product of scientific knowledge. The interaction of the individual with others is an accidental or practical necessity for scientific activity but surely, in principle, science could be done by a lone computer of sufficient intellectual or logical capacity. In spite of this basically hostile view toward social factors prevalent among scientists, there is an uneasy awareness that all might not be well with the folklore version of science. This is reminiscent of the revulsion which was once experienced that Newton, the embodiment of the age of reason, had shown

a genuine and prolonged interest in subjects as irrational and mystical as alchemy and an unorthodox interpretation of the Book of Revelations. John Maynard Keynes (1951) in his essay, Newton, The Man, tells us that Newton had kept these and other unorthodox writings in a large large box and that:

> "After his death Bishop Horsley was asked to inspect the box with a view to publication. He saw the contents with horror and slammed it shut. A hundred years later Sir David Brewster looked into the box. He covered up the traces...."

Much of the scientific community is genuinely uncomfortable with what they see as a dark side of science. This is especially true of those engaged in the routine, less creative aspects of science. Today, more reflective scientists do not subscribe to such an uncritical view of science. Still, there is a strong tendency to maintain a separation between the social and the intellectual. I believe that this is one of the reasons Medawar (1979) finds the Popper-Lakatos school an appealing matrix within which to anchor the scientific enterprise. No matter how tangled and messy the path to scientific knowledge may be, the end product is objective and pure (that is, rational).

We can find a loosening of this "super" rational and objective representation of science already in the work of Mary Hesse who has argued for the influence theory has upon what are taken to be observed facts (1980) and, as a phenomenon associated with this interdependence, for the network theory of knowledge (1974). Heisenberg (1975) assigned great value to the importance of tradition and personal relations in theory development. MacKinnon (1980) claims both were important for

Schrödinger's formulation of wave mechanics and its interpretation. Certainly these authors are still very much in the rational camp.

Much more explicit acknowledgement of the essentially sociological aspects of science - often associated with the "strong program" as I understand it - has been given by several authors. Bruno Latour and Steve Woolgar (1979) have focused on: the production of order out of disorder, largely by the process of the selection, as Leon Brillouin (1964) put it, of the relatively few facts we choose to explain with a scientific theory; the construction of phenomena and facts, which are then given objective, independent existence; and, most important it seems to me, the goal and end product of a laboratory or research group being papers by which the group collectively and the scientists individually "purchase" credibility. A major determinant for the individual scientist is the market for this credibility and the cost of raising equally probable alternative explanations to what one already understands and of going outside an already constructed system. This is all consistent with Medawar's (1967) representation of the job of scientists as solving problems (as opposed to simply working on solving problems). This success is then used as a warrant for getting that scientist's ideas into discourse, which is the aim of scientists. Ziman (1978) sees this discourse and argument as the mechanism by which we produce reliable knowledge. Certainly the matter of reliability is problematic.

I would rather say that an idea or a theorist must enter the field and hold it against all comers simply because this is the best

way we have of producing stable knowledge. That is, the creators of this knowledge and the most able critics in the field have asked all the probing questions they have been able to think of and the theory (if successful) has survived. So, of course, such a theory is stable (for a longer or shorter period of time), until some more clever person finds the crucial chink. This stability is related to the fact that very few people have the ability and good fortune to create a theory which can cover a set of data and keep them nailed down while the theory is adjusted to cover some new phenomenon. I (Cushing, 1982a, p.78) feel that a case can be made for scientists as clever people who make their theories work, rather than as discovering laws of nature which preexist outside their own minds. Of course, not just any theory can be made to work. Theorists generate a network which covers reality. This is in some sense analogous to a suit of clothes which fits a person. There is certainly no unique suit which is the only one that fits. However, a bad fit is evident. Perhaps the success of a theory resides as much in the cleverness and continued ingenuity of its creator as in its external, objective status as a "law" of nature. A similar view of successful philosophical systems or explanations put forth by pugnacious and brilliant philosophers who argue well, write extensively, and bludgeon their opponents into submission (or at least silence) would possibly not raise great objection in some quarters.

Andy Pickering (1982) has attacked the "discovery fairytale" in modern particle physics and made instead a case for this process of construction, in which modeling by formal analogy is a central feature.

In a brilliant and exhaustive essay-really a book- Constructing Quarks, he (Pickering, 1981) has analyzed the development of quark model field theory in terms both of the interest of the individual scientist in exploiting his own acquired expertise (of some method or technique) in a given field and of a recycling of analogies. His recycling of expertise is a very plausible mechanism in terms of what one sees going on among his fellow scientists. The options a theorist has open to him are nearly defined by the technical skills and knowledge he already possesses. A theory not only determines the experiments to be done but also heavily biases the interpretation of the subsequent data. Early data showing parity violation (Franklin, 1979) and others indicating the existence of the Ω^- (Pietschmann, 1976) are just two examples of phenomena which were not "seen" until the proper theory had been created. Subsequent experimental results are then used as confirmation of the theory. This raises the genuine question of whether or not such complex and open-ended modern theories can ever really fail. Even granting that there are in science norms external to the individual, these norms, as Harriet Zuckerman (1978) has observed, have themselves been constructed so that they need not be objective (or super-rational). Helmut Kirchner (1982) has recently discussed the influences of fashions in physics.

To keep the discussion from remaining completely in the realm of opinion, I now turn to several specific examples of creative work in modern high-energy physics. From these examples I shall then attempt to draw some general philosophical conclusions related to the observations

I have just made and shall also return to the realism question.

II. ELEMENTARY PARTICLE THEORY

I begin by summarizing some of the major developments in quantum field theory before turning to S-matrix theory. A review of quantum field theory can be found in Weisskopf (1981) and of both the quantum field theory and S-matrix theory research programs in an article of my own (Cushing, 1982a). The following examples of creative moves made by theorists comes quite directly from my previous paper (1982a). I have decided to concentrate mostly on field theory because many of you may be more familiar with that than with S-matrix theory.

In ordinary quantum mechanics, operators are associated with observable quantities and those operators corresponding to canonically conjugate variables, such as the position q and the momentum p, are taken to satisfy the canonical commutation relation

$$[q,p] = i\hbar$$

Dirac (1927) gave the first detailed, complete, and coherent treatment of the interaction between a quantized nonrelativistic atom and a quantized electromagnetic radiation field. He exploited the formal analogy between the free electromagnetic field and a set of uncoupled harmonic oscillators to quantize the electromagnetic field. In this same paper he introduced what have become known as the creation and annihilation operators of modern quantum field theory. At this time, however, Dirac pictured the photons as making a transition from an unobserved ground state, containing infinitely many photons, to the actual observed state under consideration, rather than in terms of the photon being "created".

The infinite zero-point energy associated with this infinite collection of the electromagnetic field "oscillators" was simply defined away by being subtracted (as unobservable) from the total energy to obtain the physically measurable energy. This was the first, and least serious, of the infinities which would arise in quantum field theory. Dirac (1928) subsequently developed a relativistic quantized theory for a free electron. This theory had two interesting aspects for our purposes here. As far as surplus mathematical structure which was given a physical interpretation, it contained as necessary results of its equations the spin of the electron and an infinite set of negative energy states not bounded from below. An electron in any given positive-energy state could spontaneously cascade down into ever lower energy states so that a free electron would not be stable. Dirac (1930) overcame this by postulating a "sea" of electrons which completely filled these negative-energy states in accord with the Pauli exclusion principle. What we now think of as the creation of a positron was pictured as the transition of an already-present negative-energy electron up to a positive-energy level. The infinite but unobservable negative charge density of this "sea" is even more counterintuitive than the infinite zero-point energy of the electromagnetic field "oscillators".

Other problems of interpretation for quantum field theory soon arose. When a covariant quantization of the electromagnetic field coupled to a matter field (say, an electron) was attempted, a consistency problem was encountered for the electromagnetic four-potential $A_\mu(x)$ which was taken to be a quantized operator. Since one always has

the freedom to make a gauge transformation

$$A_\mu \rightarrow A_\mu + \frac{\partial \Phi}{\partial x_\mu}$$

without affecting the values of the physically observed electric and magnetic field ($\vec{E}$ and $\vec{B}$, respectively), he has the option to choose the so-called Lorentz gauge

$$\frac{\partial A_\mu}{\partial x_\mu} = 0.$$

The difficulty is that the relativistically covariant canonical commutation relations themselves lead to the result

$$\left[\frac{\partial A_\mu(x)}{\partial x_\mu}, A_\nu(x')\right] = i\frac{\partial}{\partial x_\nu}\Delta(x-x') \neq 0.$$

These last two equations are clearly inconsistent ($0 \neq 0$). Fermi (1929) suggested imposing the gauge condition not as an operator identity but as a subsidiary constraint on the state vector ϕ as

$$\frac{\partial A_\mu}{\partial x_\mu}\phi = 0, \quad \frac{\partial A_\mu}{\partial x_\mu}\phi = 0.$$

However, the most important consistency problem that plagued quantum field theory was the presence of infinities which eventually led to the renormalization program. Even classical electron theory has a divergence problem with the self-energy of a point electron. Calculations in quantum electrodynamics are done as a perturbation expansion in the fine structure constant

$$\alpha = \frac{e^2}{\hbar c} = \frac{1}{137}.$$

The divergence of certain terms calculated to various orders in α was to be a general feature of this theory. Oppenheimer (1930) found an infinite shift for corrections to atomic energy levels and Waller (1930) an infinite self energy for a Dirac electron. By the mid 1930's some workable subtraction procedures - or well-defined rules for throwing away certain infinite terms - had been worked out to produce finite answers. However, some infinites, such as the self-energy of the electron, remained. Unambiguous, finite answers could not always be obtained in a calculation.

Dramatic progress was made after the Second World War in improving the calculational effectiveness of quantum field theory. Lamb and Retherford in 1947 determined experimentally that two energy levels of hydrogen which should have been identical in one-electron Dirac (1928) theory were in fact separated by a small but finite energy difference. In a remarkable calculation, Bethe (1947) made a nonrelativistic perturbation calculation of the self-energy of an electron in a bound state of a hydrogen atom. The most singular term Bethe found was just that present for a free electron. Arguing that this should be included in the physically observable mass of the electron, he discarded it as a not separately observable effect. There still remained another divergence of the type already known from Dirac's hole theory. This he also discarded in the hope that a relativistic calculation would produce the same term, plus, possibly, an additional small finite correction. Bethe took the finite remainder of his calculation to be the Lamb shift.

His calculation produced a result within about 4% of the experimental value. The extreme accuracy of these measurements becomes clear when we appreciate that the energy shift measured by Lamb was a correction of about one part in ten million to the original energy level itself. Since the Lamb shift remains one of the showpieces quantum electrodynamics, let us summarize the present status of the experimental and calculational results.

experimental: $\Delta E = 1057.77 \pm 0.01$ Mc/s

theoretical:		
	self-energy	1011.45
	vacuum polarization	-27.13
	vertex modification	67.82
		1052.14

Bethe (1947) in his initial calculation had estimated only the self-energy modification which, it turns out, gives the overwhelmingly major part of the contribution. The theoretical correction above is calculated to order α. The next nonvanishing correction is of order α^2 and if this is included the net predicted effect becomes 1058.03 Mc/s.

Kramers (1950) pointed out at the 1948 Solvay Conference that observable effects calculated in quantum electrodynamics depend upon the charge e and mass m only as measured experimentally. At this time theorists were able to do some higher order corrections by subtracting out (that is, really just throwing away) divergences as they occurred in the calculations. However, the answers obtained, while finite, were not unique and the entire procedure was a delicate and ambiguous one. This technique developed into the modern renormalization program of quantum electrodynamics which consists basically of the following. In doing a

perturbation correction to first order in α, one obtains two types of divergent integrals, one of which can be associated with an electromagnetic mass δm which modifies the "bare" mass m_0. One simply takes the (finite) physically observed mass m to be the sum of the bare and electromagnetic masses

$$m = m_0 + \delta m.$$

Both the bare mass m_0 and the electromagnetic mass δm are infinite, while the physical mass m is finite. Only the physical mass m appears in calculated quantities which correspond to physically observable processes, but never the bare mass m_0 nor the electromagnetic mass δm separately. Similarly, to first order in α another divergent term occurs which again appears only in a suitable linear combination with the "bare" charge e_0 of the electron. This can be interpreted as a modification of the charge due to (vacuum) polarization effects and the combination is taken to be the physically observed charge e of the electron. Here, too, neither the "bare" charge e_0 nor the polarization term appears separately, but only the combination making the physical charge e. Therefore, to first order in α, charge and mass renormalization may be employed to make the calculated theory finite. It is important to realize that this first order procedure can be carried out in a completely Lorentz covariant, and therefore unambiguous, fashion and that these results agree extremely well with experiment. The crucial question now becomes what happens when higher-order terms in α are calculated in the perturbation expansion. Divergent terms again appear, but they can all be written in terms of those of mass and charge renormalization, and no others. In fact, to every order in α, the same procedure of mass and charge renormalization will remove all infinite quantities, leaving finite corrections. This is a remarkable circumstance and is necessary for quantum electrodynamics to be useful as a calculational tool. If new types of divergences were to occur in each successive order of perturbation, then, even if we could associate each of these infinities with a renormalization or redefinition of various physically observable quantities, the theory would have no predictive power. Any field theory for which a finite number of redefinitions is sufficient to remove all the divergences to all orders of perturbation theory is termed renormalizable. A concise statement of any renormalization program has been given by Matthews and Salam (1954) as consisting of three steps:

1. the number of different types of infinities must be finite;
2. a subtraction procedure must be found to remove all the infinities;
3. it must be shown that the procedure holds to all orders in perturbation theory.

Weisskopf (1981) has recently written an article whose recurrent theme is the "battle against infinites." While this _is_ a dramatic expression, it is important not to lose sight of the fact that these infinites which have been encountered do not exist in _nature_ but rather in a _theory_ (quantum field theory) constructed to represent nature or reality. His story is one of the ingenuity of theorists in overcoming or isolating this trouble with the theory, not with nature.

Nevertheless, it remains clear that, because of these infinities,

quantum electrodynamics cannot be claimed to be a mathematically consistent theory. There had been some hope that the infinities which arose were a result of the approximate nature of the perturbation calculations rather than of the theory itself. As a rather naive analogy, consider the following power series expansion for $x > 0$

$$e^{-1/x} = 1 - \frac{1}{x} + \frac{1}{2!x^2} + \cdots$$

In the limit $x \to 0^+$, every term in the series (except the first) diverges, but the exact result is zero. However, that such is not the case for the renormalization constants of quantum electrodynamics was shown in 1953 by Källen. Independent of any perturbation theory, he argued that in an exact formulation not all of the renormalization constants of quantum electrodynamics can be finite.

Even if a theory is renormalizable, though, this does not necessarily make it useful for calculations since the renormalization program and the calculations themselves can be carried out explicitly only within the framework of perturbation theory. For quantum electrodynamics this is not a problem since the expansion parameter (or coupling constant) $\alpha = 1/137$ is small and the first few terms in the series might reasonably be expected to give a good approximation (even if the series itself should only be an asymptotic one). However, in field theories relevant to nuclear and elementary-particle physics, the coupling constant is of the order $g = 15$, so that a perturbation expansion is useless to obtain numerical results. These difficulties for strong-

interaction theory were amply apparent by the early 1950's. The numerical results were in terrible disagreement with experiment.

Quantum electrodynamics was the only really successful quantum field theory and as such provided a model or analogue for further theoretical developments, or, we might say, for creative moves. We have already mentioned that this theory allows a gauge transformation to be made. This feature turned out to be a key to future progress. The basic idea used in modern gauge field theories was put forward in 1954 by Yang and Mills. Although their argument concerned isotopic spin conservation, the line of reasoning is most directly explained for a simpler case. In classical particle mechanics any invariance or symmetry of the equations of motion implies the existence of a conserved quantity. For example, if the interactions among a system of particles are such that the form of the equations of motion is left invariant under a rotation about some axis fixed in space, then the angular momentum of the system about that axis is conserved. Similarly, in a field theory, Noether's theorem (1918) guarantees that, corresponding to any transformation or invariance group which leaves the equations of the theory invariant, there exists a conserved quantity or constant of the motion (that is, a quantity whose time derivative vanishes by virtue of the equations of motion). As an example, the nonrelativistic Schrödinger equation is left invariant under the phase transformation of the wave function $\psi(x)$

$$\psi(x) \to \psi'(x) = e^{i\lambda}\, \psi(x)$$

where λ is a constant. This invariance yields conservation of probability or of charge. Such a phase transformation is known as a global gauge transformation since, once λ has been chosen, the phase is fixed at every space-time point (that is, globally). Essentially what Yang and Mills argued was that this unobservable phase should be able to be chosen arbitarily at each space-time point x. This requires local gauge invariance

$$\psi(x) \rightarrow \psi'(x) = e^{i\lambda(x)}\,\psi(x)$$

where $\lambda(x)$ is now an arbitrary function of the space-time variable x. However, the equations for the coupled electromagnetic field $A_\mu(x)$ and the electron wave function $\psi(x)$ are no longer invariant under such a phase change unless the electromagnetic four-potential $A_\mu(x)$ is simultaneously subjected to the transformation

$$A_\mu(x) \rightarrow A'_\mu(x) = A_\mu(x) + \frac{1}{e}\,\frac{\partial\lambda(x)}{\partial x_\mu}$$

In the context of electromagnetic theory, this is just Weyl's (1931) gauge invariance.

Yang and Mills were able to show how to build into a theory a general symmetry group by use of such local gauge fields. The only problem was that each gauge field had associated with it a massless spin-1 particle, just as the electromagnetic potential $A_\mu(x)$ has the photon. This simply produced too many massless particles which were not found experimentally. For well over a decade, the Yang-Mills theory was considered an interesting but essentially useless curiosity. It is

worth noting that Yang and Mills' motivation for introducing such local gauge transformations was an abstract or purely theoretical one, not really required by any empirical anomaly.

These ideas would eventually be applied to the field of weak interactions where the original Fermi (1933) theory had proven unrenormalizable. The weak interaction is responsible for the decay of the neutron into a proton and an electron

$$n \rightarrow p + e^- + \nu$$

The problem was to find a renormalizable theory of the weak interactions. The resolution to the massless gauge particle problem was provided by Higgs (1964) who found a mathematical mechanism, known as spontaneous symmetry breaking, which can turn the initially massless particles of a theory into massive ones.

In the late 1960's Weinberg (1967), and independently Salam (1968), combined the Yang-Mills gauge fields with the Higgs mechanism to produce a unified theory of the weak and electromagnetic phenomena. An analogy between many properties of weak and electromagnetic interactions was used as motiviation. In this Weinberg-Salam model the Yang-Mills spin-1 field responsible for the weak interactions is put on an equal footing with the photon field (that is, the electromagnetic four potential) and both are initially massless, as are the electron and neutrino fields. A Higgs field breaks this symmetry, giving the electron its mass as well as the spin-1 weak-interaction boson its mass. The mathematics is so arranged that the physically observed "broken" theory has massive

spin-1 particles (the charged intermediate vector bosons), and the massless spin-1 photon, in addition to a massive electron and a massless neutrino. These intermediate vector mesons have not yet been seen in experiments. Initially it was a hope that this theory would be renormalizable. In 1971 't Hooft proved that such is indeed the case. It is precisely the spin-1 gauge theories that are the renormalizable ones. The Higgs particle plays a peculiar role in the Salam-Weinberg model since it is essential to provide the spontaneous symmetry-breaking mechanism and yet, because these Higgs particles have not to date been observed experimentally, its mass must be made extremely high so that it has essentially no effect on many of the predictions of the theory. In principle its mass could be pushed to higher and higher values to save the theory. On the other hand, the detection of the Higgs particle would be a great piece of supporting evidence for the theory. In this sense one might say that the existence of the Higgs particle is verifiable but not falsifiable.

This fairly complete and reasonably well-confirmed unification of the weak and electromagnetic interactions has prompted an attempt to unify these with the strong interactions in a grand unified field theory (or GUT in the trade). One first attempts to build a gauge theory of strong interactions. The basic ingredients in such a theory are quarks (Gell-Mann, 1962) (that is, fractionally charged, spin 1/2 elementary particles), implementation of SU(3) symmetry via Yang-Mills (1954) gauge fields, the spontaneous breaking of this symmetry by the Higgs (1964) mechanism, and the requirement of renormalizability. An initial difficulty with the three quarks initially postulated, aside from their not being observed experimentally, is one of principle. In order to produce stable ground-state configurations of three quarks in a physical nucleon, one has to violate the Pauli exclusion principle which governs fermions. To avoid this Greenberg (1964) postulated what was subsequently to become the attribute of color for quarks. Each quark came in three colors (say, red, white, and blue or 1, 2, 3) so that, rather than being the three quarks expected at that time, there were indeed 3X3 = 9. This was purely an ad hoc move to save the exclusion principle. The other label for the quark (often referred to as up, down and strange) became known as flavor. Hence, in the mid 1960's there were three colors and three flavors of quarks. Every quark has a color label and a flavor label. In 1970 Glashow, Iliopoulos, and Maiani postulated the existence of another flavor (c for charm) in order to eliminate some unwanted predictions from one version of these unified theories. Speaking very loosely, cancellations of pairs of divergences can be arranged among the four quarks but not among just three of them. Subsquently, this concept of charm provided a natural interpretation of the extreme stability of certain states observed in high-energy scattering experiments. This brought the number of "elementary" particles, or quarks, to 3 (colors) x 4 (flavors) = 12.

A major difficulty which remains is that no free quarks (of which three are now 3 x 6 = 18) have ever been found in spite of extensive searches at high energies. As a result, the ad hoc move has been made to assume that all physical states consist, essentially, either of three

quarks bound together to make a nucleon or of quark-antiquark pairs bound to make a meson, so that free quarks cannot exist. This is purely an assumption appended to the theory and has never been proven to be the case in any realistic gauge theory. It is referred to as confinement.

I shall now make a few comments on another research program in high energy physics, namely S-matrix theory. My presentation on this will be rather brief since an extensive and detailed study of the program will be forthcoming (Cushing, 1982b). The origin of this approach is often referred back to Heisenberg (1943) in the 1930's and 1940's when he suggested that the breakdown of quantum field theories might be due to the existence of a fundamental length and that, as an interim program, one should base a theory directly and exclusively on observable quantities (a motivation similar to that for his own initial formulation of matrix mechanics). The formal mathematical device for implementing this program is the scattering matrix, or S matrix. Physical scattering or transition probabilities are obtained as squares of the entries in the matrix. This S matrix must be unitary

$$SS^{\dagger} = I.$$

The physical significance of this mathematical statement is that it expresses conservation of probability in the sense that if a system is known to be in some definite initial state, then the sum of all the probabilities for its appearing in any allowed final state must add up to unity. The S matrix must also be covariant with respect to Lorentz transformations and analytic (in the sense of complex variable theory) with respect to the variable on which it depends. I really cannot go

into this last property here. It is related to a type of causality. These three central properties - unitarity, Lorentz covariance and, especially, analyticity - were abstracted from specific field theory models, which were then discarded. Since the theory is formulated only in terms of the observable S matrix, the expectation is that all calculated results will be finite.

However, the actual S-matrix program of today owes little to Heisenberg's original proposals. The program became a serious, autonomous one only in the late 1950's when several developments, having their roots in quantum field theory, appeared to offer a new approach to the problems of strongly interacting particles. By this time, Heisenberg himself had abandoned his earlier program. In a model motivated by field theory, Chew and Low (1956a, b) were able to use the equations produced from this model to relate to each other certain physically measurable quantities in a way that did not depend on the details of the field theory itself. Gell-Mann (1956) seems to have been among the first to suggest the possibility of an analytic S-matrix program which might largely replace quantum field theory. A series of results obtained in the late 1950's and early 1960's, many again abstracted from field theory, lent support to the conjecture that a complete theory could be based on unitarity, Lorentz invariance, and analyticity. The key ingredient was unitarity and this led to the bootstrap conjecture by Chew (Chew and Mandelstam, 1960; Chew, 1962) according to which the S matrix would be determined uniquely and self-consistently by the equations of the theory. These equations are provided by the unitarity

condition which, when written out with all the variables, indices, summations, and integrations displayed explicity, constitutes an infinite set of coupled, nonlinear, singular integral equations. In the nearly intractable nature of this mathematical problem was seen the hope for a unique and sufficiently rich solution to describe high-energy phenomena. The idea was that a nonlinear system is highly constrained, often having no solution, but, when there is a solution, it is often unique. In contradistinction to a linear problem, where any solution can be multiplied by an arbitray constant factor and still remain a solution, the nonlinearity of an equation can uniquely fix magnitudes. Such a theory could also provide an answer to a question as to why the mass of a proton (say) has the specific numerical value it does (about 1.67×10^{-24} gm). In quantum mechanics or conventional field theory, such parameters must be given as input (taken from experimental data) and cannot be explained or produced as output by the theory.

There were several impressive achievements of S-matrix theory in the early days, but the program was eventually abandoned by many of its workers since the mathematical equations proved intractable. The theory did not make incorrect predictions, but simply not enough predictions. Several of the theorists who had made key contributions to the S-matrix program now became participants in the gauge field theory enterprise. However, recently, through an ingenious use of topology as a new feature of the S matrix, Chew (1979) and his coworkers have greatly increased the empirical content of the program. They are able to account for several general phenomenological rules which previously had

no explanation (either in field theory or in S-matrix theory), to produce numerical predictions for experimental cross sections, and to obtain many of the general results or features which had been peculiar to gauge field theories - even "quarks", although of a rather different kind. But, that is a long story in itself. The complexity of the mathematics is still a major barrier, as well as being a continuing source of new developments.

III. CONCLUSIONS

Let me recapitulate the main features of this story as I have told it. There have been repeated moves to save a theory from contradiction. A few examples from quantum field theory are:

1. Dirac's negative energy sea of electrons
2. Fermi's interpretation of the gauge condition as a subsidiary requirement on the state vector
3. the subtraction of infinite zero-point energies and of infinite vacuum polarizations
4. the renormalization program
5. spontaneous symmetry breaking and the Higgs mechanism
6. the introduction of color
7. the introduction of various flavors, such as charm
8. the assumption of confinement to account for the absence of free quarks

A similar list can be made for S-matrix theory but, due to the limitations of time, I shall concentrate mainly on my field theory examples.

The use of abstract mathematical models is evident in all this as is the exploration of the surplus structure of these models - monopoles and solitions in gauge theories, topology in S-matrix theory. There is a predomiance of mathematics as the source of new physical concepts - charm (from the suppression of unwanted terms), the group-theory origin of quarks, the Higgs boson, the topological "quarks" of S-matrix theory. There has been a recycling of ideas as a result of individual workers recycling their expertise. This is especially evident in the way that most theoretical physicists are tied to their experience and technical skill in working with Lagrangian field theories (Chew, 1971; Pickering, 1981). It is also evident for theorists (such as Mandelatam) who have shifted from the S-matrix program to the field theory one, as well as for those (such as Balázs) who have stayed with the S-matrix theory. The composite model picture has gone from atoms, to nuclei, to quarks (and even, possibly, beyond). The analogy of quantum electrodynamics as a guide for gauge field theories, such as quantum chromodynamics, is apparent. Experiments and observed phenomena make sense and have meaning only within the framework of a theory which selects "meaningful" events to study. That the order "seen" is a function of the selection made is indicated, for example, by the fact that quantum chromodynamics and topological S-matrix theory are valid (in the sense of being calculationally adequate) in different domains of reactions (often characterized, respectively, as large vs. small values of the transverse momentum). A calculationally successful theory, such as quantum chromodynamics, so defines the language of scientific discourse that even a

radically different competitor, such as topological S-matrix theory, is cast in terms of that language.

These are some of the reasons why I have suggested that great theorists are clever people who (within rather broad limits set by data, which after all, are selected) make their theories work (as opposed to being fortunate enough to discover a "law" already there). There are lots of pieces, some of which can be fit together into a workable theory to create a picture of the world as we see it. I would very much like to digress into the related issue of the motivations of the theorists in terms of a desire to create something permanent (Einstein, 1954) and of a sense of obligation (Whitrow, 1973), but once again time does not allow this now.

I conclude with a few comments - questions, really - on the problem of the reality of the central entities in a scientific theory. Ernan McMullin, among others in the audience, will no doubt set me straight very shortly on the error of my ways here. If, by creatively stretching and fitting models, theorists generate a network which they fit over the contours of the external world as they see it, the question naturally arises of the reality of the theoretical entities in such descriptions of nature. As one case in point, consider the picture which quantum field theory gives us of the "vacuum" (aether ?) as seething with particle-antiparticle pairs of every description and as responsible for spontaneously breaking symmetries initially present in the theory. Is nature seriously supposed to be like that? Perhaps we have a little more perspective today in questioning Newton's absolute

space or Maxwell's electromagnetic aether. The modern reader finds incredulous the absolute certainty with which Maxwell argued for his aether. In a long article on the aether for the ninth edition of the Encyclopaedia Britannica Maxwell concluded by discussing the vortex lines which would have to exist for an indefinitely long period of time in the neighborhood of a permanent magnet. Since any known fluid which can have vortices must also be viscous so that the vortices must ultimately dissipate as heat, he had to admit that the aether possessed another peculiar property. Nevertheless, he was adamant about its existence and even hinted at a function for the aether beyond the realm of mere physics.

> "No theory of the constitution of the aether has yet been invented which will account for such a system of molecular vortices being maintained for an indefinite time without their energy being gradually dissipated into that irregular agitation of the medium which, in ordinary media, is called heat.
>
> "Whatever difficulties we may have in forming a consistent idea of the constitution of the aether, there can be no doubt that the interplanetary and interstellar spaces are not empty, but are occupied by a material substance or body, which is certainly the largest, and probably the most uniform body of which we have any knowledge.
>
> "Whether this vast homogeneous expanse of isotropic matter is fitted not only to be a medium of physical interaction between distant bodies, and to fulfill other physical functions of which, perhaps, we have as yet no conception, but also, ...to constitute the material organism of beings exercising functions of life and mind as high or higher than ours are at present, is a question far transcending the limits of physical speculation." (Niven, 1890).

Maxwell's basic article of faith necessitating a material aether was that ultimately all forces must be forces generated by direct contact: a vis a tergo (a shove from behind), as he termed it.

The realism of theoretical entities - such as the aether - generated in the creation of theories seems implausible, except in some general sense of a quite loose and nonunique correspondence with the elements of the external world. This lends support to Laudan's (1981) recent attack upon realism. Actually, Putnam's (1975) earlier "miracle" (in the absence of a realist interpretation) would be better stated not that successful theories can be created by trial and error but rather that scientists are willing to work so long and hard on such an enterprise. One reason for the success of the theories of high energy physics discussed in this talk may lie in their being so complex and open-ended that they can never be fully explored and hence have the practical flexibility which allows them to be stretched to cope with new phenomena.

Of course, we seek more from our theories than empirical and calculational adequacy alone. They give us a stable means of organizing and comprehending our world in terms of the vocabulary they define and through an interpretation of the central terms in those theories. However, we have no warrant for assigning truth or reality to these constructs with any meaning other than that they work (or, really, haven't failed us) in fulfilling these functions. They are true only in this very pragmatic sence. I take it this is consistent with the spirit of Ed MacKinnon's distinction between ontic and semantic truth (1979) and his discussion of the meaning of truth in science (1982), with Arthur Fine's (1982) natural ontological attitude, with Horwich's (1982) separation of realism into three categories, with Putnam's (1982) recent

work, and with van Fraassen's (1980) position. We may want more from our theories, but this is the best we can do.

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